

**Structures, States, and Collectivities:
Conceptual Issues in Comparative Historical Inquiry**

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The paper provides a close analysis of current efforts at comparative economic and social history, focusing on comparisons across Western Europe and East Asia. The scope of economic history has enlarged from study of “the” industrial revolution to study of the development of European economic institutions over a millennium, to comparative study of economic and political institutions in Western Europe and East Asia. What conceptual decisions must be made in order to carry out the meso-level comparison across civilizations? What ontological presuppositions are made? What kinds of distinctive new insights are available through this approach? To what extent are there important similarities of structures in the several contexts? In what ways are these histories “sui generis”? Are there causal mechanisms of development that can be identified in England, France, and China? How can comparison be effective in establishing the basis for alternative narratives in explaining the economic and political development of Eurasia?

Particularly important contributions to “Eurasian comparison” have emerged in the past 10 years. I think particularly of Bin Wong’s *China Transformed* (Wong 1997); the “involution-revolution” debate involving Kenneth Pomeranz, Philip

Huang, Robert Brenner, and others (Pomeranz 2000), (Huang 2002), (Wong 2003), (Li 1998), (Li 2002), (Pomeranz 2002), (Huang 1990; Huang 2002), (Brenner and Isett 2002); and James Lee et al's ambitious efforts to perform a large study of historical demography across the full expanse of Eurasia in the Eurasian Population and Family History Project (*Life Under Pressure: Mortality and Living Standards in Europe and Asia, 1700-1900*; (Bengtsson and EurAsian Project on Population and Family History. 2004), (Lee and Campbell 1997)).

Particularly important recent controversies include—

- What are the most meaningful units of comparison in considering Eurasian economic development—nations or regions?
- How did rural real wages compare in England and China in 1600 and 1800?
- How did agricultural productivity compare in England and China—considering both land productivity and labor productivity?
- What were the trends in real wages and agricultural productivity in England and China? Were wages and productivity falling in China while rising in England?
- Was the Chinese agricultural economy in a process of “involution”, in which population pressure forced farmers to expend ever-higher levels of labor to maintain subsistence?
- What were the factors—institutional, environmental, international—that accounted for the “great divergence” between Western Europe and East Asia in the 19th century?

- Were there distinctive demographic regimes in Western Europe and East Asia, leading to different population dynamics and different patterns of economic development?
- Did “proto-industrialization” function as an important cause of modern economic development—and did it recur in Western Europe and East Asia?

Others have engaged the empirical issues deeply. What I aim to do in this paper is to single out some of the most difficult theoretical and methodological issues that arise when we undertake comparisons of this scope. I agree with Bin Wong in the view that comparative economic and political historical research has the potential for shedding significant new light on even parts of the story that we had thought we understood well. By witnessing parallels and divergences across the economic institutions and outcomes of Eurasia, we can re-evaluate our claims of causal centrality for some processes (e.g. secure property rights, institutions of the market, proto-industrialization) while giving new emphasis to others. But in order to achieve these gains it turns out that there are substantial issues that must be confronted--how to define the units of comparison, what to look for by way of causal mechanisms, and how to establish meaningful comparisons across institutions and cultures at opposite ends of Eurasia.

The paper will develop the central point that Wong and Pomeranz both insist upon: the model of Western development does not establish a “natural” “paradigm” instance of modernization, development, or historical change. And the theories of social, economic, and

political change that have emerged from the study of the Western experience(s) are flawed insofar as they suggest that there is one preferred or most natural pathway of development. I will also raise what seems to me to be a fundamental underlying insight: that there are alternative historical pathways to modern sustained economic growth.

I will focus on two large domains of controversy: involution versus revolution in East Asia (Pomeranz, Huang, Brenner, Li, Wong) and James Lee et al on the "community-based" approach to large comparison (as opposed to nation-based) with regard to demographic behavior and outcomes.

Structure of the problem

What is the scientific purpose of comparative economic history? Let us begin by putting the problem schematically. In the economic history of Europe or Asia we observe, at the macro-level, a number of important processes over the course of 50-100 year periods of time: changes in levels of productivity and output in agriculture and manufacture, population change, urbanization, technology change, intensification of trade and commerce, social differentiation, patterns in the standard of living, changes in the organizational capacity of the state (fiscal, regulatory, military), and changes in the organizational forms through which human activity is coordinated and controlled (property systems, family norms, business forms). Some of these social properties show a direction of change that interests us—for example, sustained growth in population or national income; sustained technological innovation; more intensive use of land or transport; sustained transition towards a more urbanized population; etc. We are stimulated to ask some causal and institutional questions: are there social factors that cause these patterns of change? Are there causal

interconnections among some of these processes? In other words, we would like to use tools of historical analysis to allow us to discover some of the causal relations that exist among institutions, environmental factors, and economic and political outcomes. We can pursue these questions by offering detailed analysis of a single case; or we can consider a small number of relevantly similar cases and see whether the tools of comparative historical inquiry can shed light on these causal processes.

Schematically, we can describe the challenge of comparative economic history as consisting of three large topic areas: (1) descriptive and factual research to determine with some accuracy the quantitative levels of crucial variables: population, real wage, agricultural productivity, inter-regional trade, handicraft industry, etc.; (2) historical investigation of the economic, political, and social institutions that were embodied in various places at various times; and (3) formulation and testing of causal hypotheses about patterns, differences, and inflection points in the experience of economic development in different settings. Briefly—what was the experience in England, China, or Japan, and why did these settings show the patterns of development that they display? How did the institutional setting, the available forms of technology and technique, the particular environmental opportunities and constraints that were present, and the activities of purposive human actors within the context of these factors lead to the patterns of development, stagnation, and change that are to be found in the historical record?

Comparisons across social and historical settings naturally provoke questions of similarity and difference. Our scientific curiosity is aroused in comparative research by the possibility that (1) comparison of different settings may illustrate that

there are common processes or structures at work, or (2) comparison may lead us to conclude that the large processes that are of interest (economic development, political development, proliferation of religious ideas) are highly distinctive in their different historical settings (France vs. England, France vs. Japan, Morocco vs. Indonesia). So we can ask the question: to what extent are there **common social causal processes** at work in the historical experiences under comparison? Candidates for common social processes include: population dynamics (“high fertility regimes dampen economic growth”), property relations (“share-cropping regimes dampen technological innovation”), state institutions and policies (“predatory states inhibit business growth”). Or alternatively, we might consider whether the historical experience of different settings is highly particular; highly path-dependent; and highly context-sensitive—with the result that we would not expect to find causal regularities across cases.

A related but distinct question arising from comparative research has to do with the possibility of common institutions and structures in separate historical settings. To what extent are there **common structures** in the several historical settings (“absolutist state,” “market,” “demographic regime”)? There is the intriguing possibility that there are cross-setting similarities in certain areas of social life: a small set of solutions to the problem of collecting taxes, a small set of alternatives in the use and management of land, a small set of possible ways of organizing the human family, etc. Candidates might include: share-cropping regime, tax farming fiscal system, feudal parcelization of political/military power, market system of wage labor. At a higher level, economic historians have sometimes singled out “Protestant

ethic,” “liberal state,” “private property,” “market” as common structures that recur in a variety of settings. Comparative economic and political research can give grounds for answering this type of question as well. It can lead us to formulate more specific, perhaps more differentiated, theories of political, social, or economic institutions, and can give an empirical and historical basis for identifying some institutional forms or processes as recurring across a number of historical settings.

The other end of the “similarity-uniqueness” spectrum is also theoretically possible in comparative economic history. We might find that England, France, China, India, and Japan all had highly distinct patterns of development, with locally particular institutions and very different signatures of development over time. We therefore need to ask this question: To what extent are developments in China or Western Europe (or England and the Yangzi Delta) **unique and particular**—the consequence of highly contingent factors in the single context? This could be true in several different ways. It could be that there is very little overlap of institutions and causes in the two experiences; it might be, hypothetically, that religious and cultural factors are primary in one setting while population and property factors are primary in the other setting. Second, it could be that similar factors are in play in each case, but that, for reasons having to do with **path dependency**, the mix of primary factors is quite different in the two settings. Third, we could have a high degree of individual variation in the composition and nature of basic institutions—states, agrarian regimes, religious institutions. On this scenario, the statement that “the state” is an important causal factor in England but not the lower Yangzi core is misleading, in that it suggests that there is an important set of institutional or functional similarities defining the “states” in the two cases. But if states differ in their

institutional makeup as much as do the 16th century French monarchy and the Balinese theatre state—then the reference to the “role of the state” is probably an instance of reification rather than rigorous causal analysis.

One possible position that might emerge from sustained comparative economic history is a more “layered” approach to the question of generality and particularity of institutions and structures. It might be that high-level institutions (“state”) are complexes of characteristics and functions that are in sum unique to the setting; so the “absolutist state” or the “theatre state” is not a portable historical construct. However, if we push the analysis down a level and single out specific state functions and capabilities (revenue collection, regulation, military policy, use of technical experts, ability to project power throughout the dominion), we arrive at a set of concepts that permit genuine and fruitful comparison across historical contexts. On this approach, comparison should attempt to bring the level of analysis down a level or two in order to arrive at discoveries of similarity and difference across historical settings.

A third large question for comparative economic history is a serious question of scale and scope of inquiry in these comparisons—the unit of comparison. Perhaps the most basic feature of social phenomena is its **non-homogeneity**, over many dimensions—location, time, ethnicity, urban-rural, etc. National comparisons—England compared with France or Japan, for example—elide many of these dimensions of difference. So the issue of scale is a methodologically and substantively important question to address: What are appropriate **units of comparison**? There are a number of available possible answers: civilizational units (Europe and Asia), nations, macro-regions, meso-regions, “typical core regions”. And the answer that we give needs to be

motivated by a cluster of hypotheses about social mechanisms and systematicity within the social activities bounded by the scale choice that we have made. Pomeranz makes a sustained case for selecting comparable regional systems rather than “civilizational units” or nations (Pomeranz 7-8); he recommends comparison of England against the Yangzi region, on the ground that populations and range of activities were roughly comparable at an early period of development. So the reason for this level of analysis is a preliminary assessment that there is greater likelihood that regions at this level of aggregation will be more meaningfully comparable to those in historically separate settings; that comparable (not necessarily similar) processes will be occurring; and that there is a greater degree of systematicity and causal interconnectedness within the social system defined by these boundaries. Here the work of James Lee and his collaborators is especially relevant; because Lee et al attempt to disregard the national level altogether, and to focus on smaller human communities selected in different locations across Eurasia. Their view is that meaningful comparison of demographic behavior is best construed at the level at which people and families interact with each other, with the environment, and with larger economic and social factors—at the community level.

So far I have emphasized some of the causal and structural issues that arise in comparative research. But we need also to pay attention to the methods of analysis that are available to the comparativist. How can we arrive at good analysis and explanation of large processes like economic growth, political development, or urbanization? What are some of the **tools of comparison** through which we can arrive at better causal and historical analysis and illuminate the concrete historical processes underway in both regions or settings? These are questions that have been at the center of discussion in a

recent burst of fine work on comparative history (Mahoney and Rueschemeyer 2003), (McDonald 1996), (Geddes 2003), (Goldthorpe 1997), (Lichbach and Zuckerman 1997), (Hedström and Swedberg 1998). But it is possible to draw a few observations from the practice of the authors under consideration here as well.

The historian interested in explaining, understanding, and comparing the economic development trajectories of Western Europe and East Asia has several important tools at his/her disposal. First, it is important to have the best possible grasp of the empirical and factual experience of the two settings. The specialized economic history literatures of Europe and Asia provide factual evidence at a number of levels of scale: national economic performance, regional or occupational living standards (wages and prices), levels of trade, levels of transport, levels of output and output-per-unit, and so on. These sources do not “speak for themselves”; the comparativist needs to exert himself in understanding the measurement issues that underlie the data, the regional variations that exist, etc. But general economic and social variables can be observed through existing economic history research at a variety of levels of aggregation. Economic historians such as Robert Allen have done highly valuable work in attempting to provide meta-level summaries of some of these data sets, to provide the strongest basis for comparison across contexts; so Allen’s detailed summaries of cost of living and wages across Europe and Asia provide a significantly more rigorous basis for cross-regional comparison (Allen 2002a, 2002b, 2004); see also (Goldstone 2002).

Second, a great deal of institutional detail is known about state policies, taxes, property systems, labor, etc., in a wide variety of settings across the two continental systems. Once again, this sort of historical knowledge can be gathered and aggregated at

a range of levels, from the local to the regional to the national or continental; it is a substantive issue that the historian must address in choosing the “right” level of aggregation for the purpose of the current comparison.

This point underlines the importance of further **empirical and factual research** in resolving these hypotheses and debates about economic development. And there are significant conceptual problems that need to be addressed as historians attempt to formulate summaries of the facts about population, output, productivity, standard of living, or real wage. Goldstone emphasizes the importance of discussing and motivating a set of data consistency standards that ought to guide the use of historical economic data. Goldstone's points about micro and macro data and the need to tie them together to establish consistency (Goldstone 2002). Allen's care to establish consistent accounting for measurement and comparison provides an admirable model of rigor.

Third, there are existing causal theories or hypotheses that have been developed through past economic history, and that can be investigated and evaluated in light of future comparative research. Such hypotheses include Malthusianism and population-centered theories; theories of the inhibitory and catalytic role of the state in stimulating economic growth; the role of trade; the role of colonialism; the role of technology change; etc. These hypotheses provide a role of **social theory** in comparative history, since social theory represents the zone in which the causal logic and institutional workings of such factors can be worked out.

Fourth, causal reasoning based on the logic of causal assertions (necessary conditions, sufficient conditions, causal relevance, INUS conditions) permits us to test causal assertions by examining multiple cases in which putative causal conditions and

outcomes occur. Generalizations of Mill's methods lie at the heart of Theda Skocpol's comparative method (Skocpol 1979); Brenner makes similar arguments about causal hypotheses concerning economic development (Brenner 1976, 1982); and it is possible to adapt probability theory to the assessment of small-N comparisons as well (Little 1998).

How much **causal analysis** is possible through this kind of comparative research? There are a number of causal methods of inquiry available to the comparative researcher, and they are fully applicable to the subjects that arise within Eurasian comparisons.

Several of those methods include—

- Process tracing and identification of specific causal mechanisms
- Generalizations of Mill's methods of difference and similarity
- Discovery of conjunctural causal networks
- Discovery of concrete social mechanisms; testing of resulting hypotheses across cases

What these methods are unlikely to do is to provide support for the highest-level causal generalizations that have often been advanced in economic history: “population growth causes economic stagnation,” “technology change causes economic growth,” “efficient property relations cause economic growth.” These causal statements are at too high a level of abstraction; instead, we need to be searching for a more disaggregated set of causal factors, at a level where population, state, property system, technology, and culture can all be incorporated into a causal analysis of a specific historical experience.

Finally the comparativist is in a position to offer and evaluate substantive causal narratives of the different historical experiences under review through “process tracing”,

and he/she is in a position to assign causal priority to various factors. This is the heart of Wong's findings in *China Transformed*: by examining the specifics of Chinese and European development, we can sort out causal priorities among factors that have been taken to be determining (Wong 1997). This approach gives strong support to an emphasis on contingency and path-dependency of economic and political development. There is no single pathway to development; rather, different combinations of political, property, and market institutions can lead to economic growth and development along substantially different historical trajectories.

With a good grasp of the factual and institutional circumstances that characterize the different historical experiences, the comparativist is now in a position to invoke a set of rigorous methods for comparison and inference. If the causal hypothesis is that a certain kind of fiscal regime has a certain kind of economic development effect, the comparativist can systematically review a collection of cases in which the fiscal regime is present or absent, and consider what the effects are in these different settings.

What do we hope to know as a result of these sorts of comparisons?

What is the ideal product of a successful comparative economic history of development? We would like to have a compact but non-reductionist analysis of the causal properties of a family of social, political, and economic institutions with regard to their importance and weight in influencing economic change. (The causal properties of an institution include the typical and cross-context effects the institution has, and should be supported by a theory of the mechanisms that bring about these effects.) We would like to have an account of the most important causal mechanisms that arise in economic

development, and we would like to be able to construct narratives that illuminate the similarities and differences in the several regional experiences that we have considered.

If we have the good fortune of finding that there is a relatively small number of factors that have prominent effects on the pace of economic development, we would hope to be able to support some causal generalizations across cases. For example,

- Secure property systems enhance technology innovation
- Improvements in transport efficiency cause economic growth
- Increases in mobility of labor cause rising urbanization
- Social insurance regimes increase economic growth

The knowledge that emerges from comparative research should provide answers to questions about **contingency and necessity** among large economic and political developments in East Asia or Western Europe. Do we find that economic outcomes reflect a large degree of path-dependency and contingency? Do we observe substantial variation in the basic institutions through which economic and political functions are performed? Or is there convergence around “most efficient or effective” political and economic institutions? The results so far suggest a high degree of variation and contingency across Eurasia.

At the same time, we would hope that the results of historical comparison will shed light on the degree of systematicity or causal order that exists in economic and political development processes. There is variation, but there is also a degree of causal order. So each economic history is not *sui generis*; instead, productively, we find that there are causal similarities and contingent differences.

Contingency and necessity

An important lesson that emerges from these comparisons is the importance of multiple feasible pathways and institutional settings for economic and political development. Wong, Lee, and Pomeranz show that there is ample room for contingency and agency within the historical experience: there were historically feasible alternatives in the organization of production with modern technologies; and in fact, managers, workers, and planners exploited these contingencies so that the alternative forms in fact prospered in various settings. This narrative conveys less of a sense of iron necessity in outcomes, relative to the standard approach to the history of industrialization of Europe.

The detailed scrutiny of these forms of contingency and diversity is highly productive. It emerges that the economic and technological changes since the Industrial Revolution have brought with them enormous evolution in the types and complexity of the organizations through which economic and technological activity takes place: for example, enterprises, banks, electric power networks, factories, engineering and architectural firms, research institutes and laboratories, regulatory agencies, producers' cooperatives, manufacturers' associations, and labor and craft unions. Modern technology systems require the coordinated actions of large numbers of skilled and knowledgeable workers and scientists; they require the coordination of multiple physical processes (flows of materials and energies); they pose the possibility of failure, large or small; and they require the smooth transmission of information and feedback throughout the organization. The flow of information within the unit; the training processes through which workers learn routine and emergency skills; the organizations through which safety procedures are implemented and monitored; the decision-making processes

through which products, services, and processes are reviewed and modified; the design process through which new products and processes are invented and designed—all these features of organization affect the success or failure of the enterprise. Different organizational forms may have very different effects on these various functions.

Complex organizations are thus both effect and cause of important impulses of economic and technological change, and different organizational types may have dramatically different effects on the efficiency, equity, or progressiveness of the complex of activities that they encompass.

Concluding observations

Comparative economic history can discover some **common middle-level processes** that recur in different settings—economic behavior, family and reproductive behavior, incentives and opportunities presented to the wielders of monopoly coercive power—and that **different institutional settings** can lead these processes to radically different outcomes. Moreover, there are interaction effects among the institutions that regulate the various common processes; thus the particulars of a given set of political institutions (designed, perhaps, to impede the ability of military commanders to challenge the emperor; Kuhn 1980) may impede development of effective financial institutions, and therefore impede the development of large-scale enterprises with large geographical scope. Peasant production—smallholding and tenant farming—may place a limit on improvements in agricultural productivity that constrain the state's fiscal capacity—and hence its ability to finance military or commercial infrastructure. Large-scale commercialization of a product sector—e.g. cotton textiles—may be so successful at producing large quantities at low price, that technological innovation is discouraged (Elvin 1973). And so forth; the general point is that **institutions**

matter, and that institutional arrangements in different sectors may impose limits (or sometimes opportunities) that discourage or favor some pathways of development over others. Instead of an expectation of one grand course of development, we ought to expect a congeries of contingent, fluctuating path-dependent processes.

One important result of study of these important current historical works is this. Let us consider China's historical development—economic, agricultural, political, social, military—in its own terms, but informed by the best available social theoretical insights and concepts; let us identify China's own “paradigms” of development, its own **pathways of political development and economic change**; and let us use those new-found paradigms to inflect our understanding of the processes of other parts of the world. Finally, let us recognize that the stuff of social theory takes us a ways down the road of being able to explain particular pathways of historical development in a variety of contexts; but it does not permit us to make confident predictions about **uniquely determined outcomes**. In place of the overtones of inevitability— population increase, technological change, improvement in agricultural productivity—we get more nuanced narratives of diversity and contingency, and the recognition that historical outcomes are under-determined by any particular and limited set of causal factors. And in fact, Wong, Lee, and Pomeranz show that careful comparative study of the economic histories of different regions of Eurasia will establish this **plasticity of outcome**. For example, Wong carefully assesses the literature on proto-industrialization in Europe; finds that very similar processes of rural manufacture are present in both Europe and China; and argues that the causes of European “breakthrough” must therefore be sought elsewhere. More generally, he argues that similar processes of commercialization and

population dynamics are associated with very different paths to (or away from) industrialization (Wong 1997:46, 47).

Several important methodological points emerge these comparative studies of Europe and China. First is a point about the **role of social theory** in historical inquiry. Wong recognizes that reliance on current social theory is inescapable in historical analysis (what else would provide the analytical basis for comparison and hypothesis?), but he emphasizes the importance of doing so with care and critical intelligence. As Susanne Rudolph puts the point, “At this stage we need fragile theoretical templates, made of soft clay rather than hard steel, that adapt to the variety of evidence and break when they do not fit” (Rudolph 1987:738). Crucially, Wong insists on the point that the researcher must be critical in extending ideal-typical concepts of structures and processes from the European context to an Asian context. More acutely, we need to find new ideal-typical configurations of institutions and processes in Asia (and other world civilizations), to add depth to our understanding of European history. Finally, Wong, like other scholars, emphasizes the **plasticity of large historical developments**. There are multiple contingent factors involved in any large historical process, and there is room for choice by agents at all points along the way.

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